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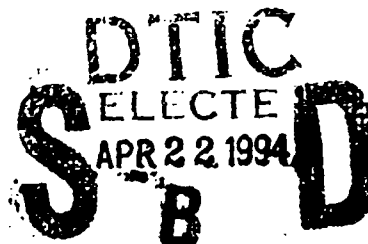
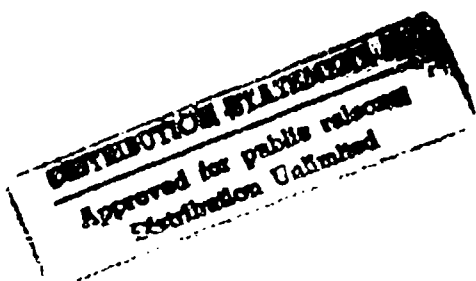


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# The National Aerospace Plane (NASP): Development Issues for the Follow-on Vehicle

## Executive Summary

B. W. Augenstein, E. D. Harris



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PROJECT AIR FORCE

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# **The National Aerospace Plane (NASP): Development Issues for the Follow-on Vehicle**

## **Executive Summary**

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A Project AIR FORCE Report  
prepared for the  
United States Air Force

# **RAND**

## PREFACE

This Executive Summary presents the final results of a Project AIR FORCE study entitled, "The National Aerospace Plane (NASP): Development Issues for Follow-On Systems." The research was approved by Major General Albert L. Logan, United States Air Force, Director of Plans, DCS/Plans and Operations, in December 1987. It provides an independent overview assessment of the NASP program, which is currently in Phase II of a three-phase Research, Development, Test, and Evaluation (RDT&E) program. The purpose of the NASP program is to develop and demonstrate hypersonic and transatmospheric (single-stage-to-orbit) technologies that will support future national security and commercial applications and provide economies in space-launch costs. Phase II is a multi-year technology demonstration program with the goal of proving that relevant technologies are sufficiently mature to support the development of an experimental flight vehicle, called the X-30.

This study provides inputs for the Air Force's determination of whether and how to proceed with Phase III of the NASP program—development of the X-30. It was conducted as part of the Technology Applications Program of Project AIR FORCE under the sponsorship of Directorate of Program Planning and Integration (SAF/AQX).

This study drew on the results of ongoing NASP applications contractor studies, the hypersonic technology maturation program, the X-30 design and enabling technology assessments, and the materials consortium sponsored by the NASP Joint Program Office (JPO). Reviews conducted by the Defense Science Board in 1987 and the Air Force Studies Board in 1988 were of substantial assistance, as was a 1988 Government Accounting Office report on the NASP program. Finally, the authors used the results of the extensive hypersonic research program conducted by the United States between 1959 and 1965 as part of SR-89774, Recoverable Booster System.

This study used information from various years, and in some cases drew on sources as recent as early 1991, in updates. NASP JPO studies of staging (which led to JPO reiteration of the merits of single-staging) were reviewed in August 1990. It is sometimes said that progress is so fast in this program that much of the information base used will be overtaken by events in a short time. The authors do not believe this to be the case for the major program observations here. In some instances, continuing issues noted in this document were first raised in 1985–1987.

Since distribution of RAND's research results to the Air Force sponsor first in draft format in November 1990 and then in final form in February 1992, the authors have continued to monitor NASP technology progress as reported semi-annually at the NASP Technology Review symposiums. The results presented at the 1992 symposium reaffirmed their view that, as of that date, no new developments have occurred that would require them to modify any of their technology assessments or conclusions, recommendations, or perspectives based on those assessments. In addition, in January 1991, the USAF Scientific Advisory Board's Panel on Hypersonics requested that RAND brief them on their NASP findings and the implications for Air Force hypersonics interests. The panel offered no negative comments on RAND's technological assessments, nor did the NASP Interagency Office object to the presentation.

The scope of this research includes RAND's findings for NASP comparative mission assessments, technological challenges, costs, environmental issues, and programmatics. The inclusion of RAND's assessments of NASP technology, costs, and programmatic issues in this report does not imply Air Force concurrence. Nor does the omission of an assessment of NASP industrial infrastructure issues, originally planned for inclusion in this study, imply Air Force concurrence. Within the resources available for this review of the NASP program, the authors concentrated on the critical issues, pursuant to providing the Air Force with an independent, integrated overview of a potential NASP-derived operational system as an input to the Phase III decision on the X-30 flight test vehicle. The NASP industrial infrastructure and logistics issues, for instance, were not judged to be as critical as technological, cost, and programmatic issues given the relatively small fleet size required to accomplish the important missions as determined by our mission and cost assessments.

This report should be of interest to those concerned with the planning and implementation of the United States hypersonic technology research program, the military space support mission, future space launch systems, and hypersonic technology for commercial aircraft applications.

Additional research for this project may be found in the following documents:

- N-3127-AF, *Soviet Reactions to the National Aerospace Plane (NASP)*, R. Gottemoeller and N. Brooks, June 1990.
- N-3253-AF, *Numerical Simulation of Hypersonic Aerodynamics and the Computational Needs for the Design of an Aerospace Plane*, S. K. Liu, 1992.
- N-3463-AF, *Aerospace Plane Sonic Boom Intensity Estimates: Review and Preliminary Assessment of National Aerospace Plane Operations*, S. K. Liu, forthcoming.
- N-3464-AF, *Aerospace Flights and Stratospheric Ozone: Review and Preliminary Assessment of National Aerospace Plane Operations*, S. K. Liu, 1992.

## ACKNOWLEDGMENTS

The authors wish to acknowledge the extensive and invaluable contributions made to this study by a number of RAND colleagues, as reflected on the title page. These colleagues provided substantive inputs through discussions, internal memoranda, and contacts with and participation in JPO and NASP contractor information exchanges and war game activities. In addition, the special expertise of these colleagues in technical, systems, and policy disciplines provided background that aided absorption and assessment of the detailed investigations undertaken by the JPO and NASP contractors.

Careful reviews of an earlier draft of this report by Carl Builder and Gerard Sears sharpened the discussion and treatment of important issues.

In addition, the authors thank other RAND colleagues whose advice and comments aided in the conduct of this study and in its documentation. Those colleagues include J. Bonomo, G. Donohue, D. Dreyfuss, R. Hess, D. Orletsky, J. Quinlivan, F. Wazzan, and B. Wilson.

The authors also wish to acknowledge the cooperation, assistance, and data provided to RAND throughout the study by members of the NASP/JPO System Applications staff, including Colonel John Fuller, Lieutenant Colonel Wayne Shattuck, Major Jess Sponable, and Mr. Terry Kasten. In addition, our study benefited greatly through the ongoing interactions that we had with NASP JPO contractor teams, including those at Boeing (Mr. Daril Hahn); General Dynamics (Mr. Armand Chappets, Dr. Nathum Krumm, and Mr. Arvel Toten); Lockheed (Mr. Sam Finch); McDonnell Douglas (Mr. Hershel Sams, Dr. William Gaubatz, and Mr. Bill MacFarlane); Pratt and Whitney (Mr. Carl Sypniewski); Rocketdyne (Dr. Barry Waldman); and Rockwell International (Mr. Robert Gulcher, Mr. Ed Brown, Dr. Don Zinn, and Mr. James Kirkpatrick).

Finally, we want to acknowledge the valuable assistance of Jerry Sollinger and Laura Zakaras in the planning, organizing, and production of this report, and Eleanor Espensen, Patrice Moore, and Millie Zucker in the preparation of the manuscript.

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## 1. INTRODUCTION

The National Aerospace Plane (NASP) program plans to develop an experimental aircraft, the X-30, to explore the entire hypersonic velocity flight range. The current Research, Development, Test, and Evaluation (RDT&E) program goal is to insert into low earth orbit a manned system with a multi-mode air-breathing engine in a single reusable stage. The NASP program's objectives include support of future national security, civil applications, and a reduction in the costs of space launch. These activities reflect the importance of NASP in the nation's future planning for aerospace activities.

The United States Air Force is currently engaged in Phase II of a NASP program funded jointly with the National Aeronautics and Space Administration (NASA). Other participants have included the Defense Advanced Research Projects Agency (DARPA), the Navy, and the Strategic Defense Initiative Organization (SDIO). The NASP Joint Program Office (JPO) has responsibility for the general management of the NASP program.

The NASP Phase II program is a multi-year technology demonstration effort with the goal of showing that the NASP technologies—including air-breathing propulsion, advanced aerodynamics, materials and structures, fuel systems, and avionics, and the overarching tool of computational fluid dynamics (CFD)—can achieve maturity adequate to support a Phase III effort that includes development, fabrication, and flight testing of the experimental X-30. The Space Council has set early 1993 as the date for judging this technological maturity.

## BACKGROUND

We begin by reviewing the early (1958-1964) NASP program. No formal history of that program has been assembled, as far as we are aware, and many of its considerations and accomplishments are not widely enough known or recognized.<sup>1</sup>

The early Aerospace Plane program, a post-Sputnik effort begun in 1958, was considerably spurred in 1959 by the request SR-89774, Recoverable Booster System. The goal was to have routine access to space, implying needs for:

- Safe and dependable orbit achievement
- Departure and return to ground bases of choice
- Minimal time to access space once decision is made
- Very high confidence of mission completion
- Payload size, weight, and configuration versatility
- Restricted operating base limitation only because of safety concerns

These need statements are just as critical and relevant today as then.

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<sup>1</sup>A significant portion of this effort is summarized by R. Hallion in *The Hypersonic Revolution: from Scramjet to the National Aero Space Plane, 1964-1986*, two volumes, The NASA History Series, National Aeronautics and Space Administration, Washington, D.C., 1989. This history should, however, be complemented with more detailed accounts of organizational sponsors and individual contractor efforts in the time period for a fuller review.

The philosophy underlying the early Aerospace Plane program remains relevant today in the modern space context; features were to include:

- Capability to develop into an operational system
- All-azimuth orbits
- Horizontal takeoff/landing at bomber bases (e.g., B-52)
- Safety constraints no more restrictive than B-52
- All stages return to home base on completion of operations
- Vehicle fully recoverable
- Vehicle fully reusable (minimal refurbishment)
- Minimum staging, consistent with required performance
- Maximum use of atmosphere (maneuvering, propulsion)

Consistent with these needs and operational features, the early Aerospace Plane program reviewed a great many vehicle configurations. After reviewing and dismissing several options still considered "exotic" today (such as use of an electrical accelerator), the more attractive remaining options extensively studied included:

1. Single-stage-to-orbit (SSTO) using air enrichment and collection, and employing chemical rockets as the final propulsion mode.
2. SSTO using a nuclear rocket as the final propulsion mode (variants using nuclear air-breathing engines were also contemplated).
3. SSTO using a supersonic combustion RAMJET (i.e., a "SCRAMJET") as the final propulsion mode.
4. Two-stage-to-orbit (TSTO) using air enrichment and collection technology, and a conventional chemical rocket for second-stage propulsion.
5. TSTO, using air-breathing propulsion for the first stage (all propellants carried on board from takeoff), with conventional chemical rocket propulsion for the second stage.
6. Same as 5, except that a nuclear rocket second stage was considered.
7. TSTO carrying all propellants on board from takeoff and using SCRAMJETS to power the second stage.
8. Use of various modes of hypersonic inflight refueling systems (HIRES) and employing a conventional chemical rocket as the final propulsion stage (one- and two-stage versions are possible).

Based on guidance from the Air Force Scientific Advisory Board (SAB) and other ad hoc teams in the early 1960s, the Air Force emphasized "first generation" options, which were the two-stage-to-orbit versions (Options 5-8, above).

Even today, it is difficult to fault the choice. In fact, the configuration selected corresponds closely to the German Sanger Project. RAND believes such a choice as a strong contender for the NASP project has much to commend it.

## OBJECTIVES OF THE RAND REVIEW

RAND's work on the current NASP program is largely described by the Project AIR FORCE research title: "The National Aerospace Plane (NASP): Development Issues for Follow-on Systems." The project was approved by Major General Albert L. Logan, United States Air Force, Director of Plans, DCS/Plans and Operations, on December 16, 1987. The RAND project was to aid the NASP JPO and others directly concerned with NASP in two major areas:

- To assist in efforts to define and validate the utility of potential missions, in support of the Phase III decision milestone. The Phase III decision concerns whether to fabricate the experimental X-30 NASP design.
- To conduct an integrated system overview of NASP and NASP-derived systems that considers enabling technologies, logistic support, military missions, economics, and political implications.<sup>2</sup>

## CONCLUSIONS

RAND's review led to a number of conclusions.

With respect to **NASP missions**, we have three major conclusions: (1) No single mission justifies the vehicle in and of itself, but (2) the potential ability of the NASP to perform many missions may be its best rationale. This aspect of the program will require additional research. Further, (3) the specific payloads and equipment associated with the missions will require a significant RDT&E program.

Our examination of the **technology** issues surrounding the program leads us to conclude that substantial progress has occurred in overcoming formidable challenges but that many problems remain unresolved. The present CFD state of the art will only allow CFD to serve as a design tool for NASP with a substantial program of testing, experimentation, and analysis to narrow the major hypersonic aerodynamic and combustion uncertainties. Also, the integrated propulsion system contains some unprecedented hurdles, and the current approach to solving these problems allows little margin for error. In our view, more conservative, less precarious approaches exist. Specifically, we would cite subscale experimentation to complement the basic X-30 program.

Definitive **cost estimates** are not possible for a program in this stage of maturity. Using only airframe costs as a proxy suggests no substantial launch cost savings will accrue.

<sup>2</sup>The scope of this research includes our findings for NASP comparative mission assessments, technological challenges, costs, environmental issues and programmatics. The inclusion of RAND's assessments of NASP technology, cost and programmatic issues in this Report is not intended to imply Air Force concurrence. Similarly, our omission of an assessment of NASP industrial infrastructure issues, originally planned for inclusion in this study, also does not imply Air Force concurrence. Within the resources available for this review of the NASP program we concentrated on the critical issues, as we saw them, pursuant to providing the Air Force with an independent, integrated overview of a potential NASP-derived operational system as an input to the Phase III decision on the X-30 flight test vehicle. The NASP industrial infrastructure and logistics issues, for instance, were not judged to be as critical as technological, cost and programmatic issues given the relatively small fleet size required to accomplish the important missions as determined by our mission and cost assessments.

Had we analyzed the NASP industrial infrastructure for such a small fleet size (i.e., about 10 vehicles), we probably would have discovered that the U.S. aircraft production capabilities would not be severely strained. To conduct such an analysis, precise specifications of the characteristics of both the proposed NASP-derived vehicle (NDV) and its payloads would be required. No adequately detailed specifications were available from NASP/JPO.

Further, concurrent development is not an attractive option for achieving an early operational capability, given the level of technical accomplishment still required for the X-30. As a consequence, we believe NDVs may not be available until after the year 2010.

The **environmental issues** associated with the NASP mirror those that surrounded the supersonic transport (SST). The best calculations available indicate a minimum threat to the ozone layer. Sonic booms and ground aircraft noise will require both planning and attention. Although the NASP does not appear to pose an environmental threat, concerns about these issues have a significant potential detrimental effect on the program. Program managers need to consider environmental concerns carefully and address them as the project matures.

Our review identified several unanswered and unresolved **programmatic issues**. Projections of technology maturation dates and risk moderation dates appear highly optimistic. Further, the current program focuses too narrowly on air-breathing SSTO. A wide range of alternative approaches should be analyzed in depth before the 1993 Space Council decision.

Finally, the technological progress on the NASP program—although significant—has not been sufficient or compelling enough to modify any of our technology assessments or any of the foregoing conclusions, recommendations or perspectives on NASP since this report was presented in draft form to the Air Force in November 1990.

## **ORGANIZATION OF THE SUMMARY**

Section 2 qualitatively compares the performance of the NASP-derived vehicle with other candidate launch systems in a specific mission, space launch. Additionally, Section 2 briefly discusses the significance of the "composite mission" utility, which may be a compelling characteristic of NDVs. Section 3 reviews two of the major technological challenges—computational fluid dynamics and propulsion—facing the program. Section 4 addresses two aspects of the costing of the program: potential reductions in the per-pound cost of space launch and its importance relative to spacecraft costs and the effect of concurrent development. Section 5 investigates the environmental impacts of noise issues and ozone depletion. Section 6 reviews the programmatic issues surrounding the NASP, and Section 7 presents conclusions and recommendations.

## **2. COMPARATIVE MISSION ASSESSMENTS**

During the course of this study, the RAND study team had the opportunity to review a variety of NDV performance analyses for specific missions conducted by the NASP JPO contractor team. For many of these cases, NDVs performed extremely well. In a few instances, the analyses considered alternative approaches for accomplishing the same mission, and for a smaller subset, the scenario context was also varied. As a result of this exposure, we were convinced that NDVs can accomplish a wide assortment of missions provided that mission-specific payload equipment is available. We were not necessarily convinced, however, that some of the design features of NDVs—single stage to orbit, rapid turnaround time, etc.—were essential for its success in the mission analyses. In addition, we did not see an assessment of the alternative approaches for accomplishing the NDV missions across a wide range of military scenario contexts. This type of comparison had not been undertaken largely because the necessary data were unavailable.

Rather than making an independent assessment of the performance analyses done by the NASP applications contractors for NASP JPO, we have attempted a comprehensive comparative mission assessment of NDV capabilities. Selection of potential NDV missions was not arbitrary. The missions represent the types that the NASP contractor community and NASP JPO have used to justify the military utility of NASP. Furthermore, in a strategy-to-task type of assessment that examined the linkage between desired U.S. military capabilities and weapon systems, we identified several mission areas that were probably important candidate missions to consider for NDVs. We also found, however, that there were many other possible alternative means by which the United States could achieve the desired military capabilities. For many of these space launch was essential, so we added it to our mission list. For the analysis reported in this section, we have assumed, optimistically, that the potential missions identified for NDVs represent the approach that the United States would follow to achieve the associated desired military capabilities. We therefore start our assessment with a "best-case" assumption about applications of NASP-derived vehicles and hypersonic technology. In addition, we assume that the NDV technology goals are realized and that mission-specific equipment needed to carry out the missions will be available.

In this section, we illustrate our comparative mission assessment approach with a largely qualitative first-order comparison of NDVs with alternative ways of accomplishing the space launch mission in scenario contexts that range from peacetime to nuclear war. Three other mission areas are discussed in Vol. 2 of this report.

### **SPACE LAUNCH**

In our comparative assessments, we adopted a matrix scoring system that displays the mission capability and favorable launch system characteristics for the NDV and alternative launch systems. That is, we have qualitatively assessed which launch systems could perform the various types of space launch mission objectives—satellite insertion, satellite refurbishment, or space rescue—and how well each was suited for it.

In this analysis, we postulated a number of types of launch systems, a space launch mission model, and four scenario contexts ranging from peacetime operations through nuclear war. The mission model includes single and multiple satellite insertion, satellite inspection/retrieval, satellite refurbishment, space station supply, and space station crew replacement/rescue. These space launch missions are shown in a matrix (Figure 2.1) along with the four scenario contexts. An "X" indicates that a specific mission is likely to be carried out in a particular scenario context.<sup>1</sup> For example, we expect that all missions would possibly be flown during peacetime, but that only the single and multiple satellite insertion missions would be flown in all four scenarios. A blank cell in Figure 2.1 indicates that we do not expect that the United States would fly a mission in that scenario.

Next we identified a set of launch system characteristics that may or may not be of crucial importance for carrying out a particular mission. Eight launch system characteristics were selected; they are shown on the left side of Figure 2.2. Phrased as questions, they provoke a "yes," "no," "probably," or "maybe" response, which then allows a judgment as to how important a specific characteristic is within a given scenario. Figure 2.2 gives an example for space war with the United States and Soviet Union homelands as sanctuaries. The shaded

Scenario	Mission					
	Single Satellite Insertion	Multiple Satellite Insertion	Satellite Inspection/ Retrieval	On-orbit Satellite Refurbishment/ Repair	Space Station Supply	Space Station Crew Replacement/ Rescue
Peacetime	X	X	X	X	X	X
Space war (U.S. & SU homeland sanctuaries):						
No space sanctuaries	X	X				X
Space stations and resupply vehicles in sanctuaries	X	X			X	X
Conventional war (no homeland sanctuaries):						
With space war	X	X				X
Without space war	X	X		X	X	X
Nuclear war (preceded by space war)	X	X				

Fig. 2.1—Space Launch Missions Considered for Various Scenarios

<sup>1</sup>In this first-order comparative analysis, we have relied on the experienced judgments of the RAND study team. Ultimately, a more detailed system comparison should consider system design and operational and cost factors. The methodology and scoring approach described herein could be applied with new assessments for the various scores and rankings that are consistent with the mission model and alternative systems, and could employ a more formal (e.g., Delphi) approach for arriving at judgments.

Launch System Characteristic	Mission					
	Single Satellite Insertion	Multiple Satellite Insertion	Satellite Inspection/ Retrieval <sup>a</sup>	On-orbit Satellite Refurbishment/ Repair <sup>a</sup>	Space Station Supply <sup>b</sup>	Space Station Crew Replacement/ Rescue <sup>b</sup>
Are men needed for on-orbit support?	No	No			No	No <sup>c</sup> / Yes
Are launch azimuth and orbit access important?	Yes	Yes			No	Yes / Yes
Is launch/landing site availability important?	Maybe	Maybe			No	No / No
Is launch rate/surge capability important?	Yes	Maybe			No	No / No
Is turnaround time important?	Yes	Maybe			No	No / No
Is launch on demand/hold capability important?	Yes	Maybe			No	Yes / Yes
Are covert orbital operations important?	Maybe	No			No	No / No
Is a large maximum payload per launch important?	No	Probably			Maybe	No / No

<sup>a</sup> Assumes that satellite inspection/retrieval and on-orbit refurbishment/repair are impractical during space war.

<sup>b</sup> Space stations and resupply vehicles are in sanctuary 

No
Yes

 If not in sanctuary, space station supply would not be a mission.

<sup>c</sup> In space war it is preferable to use an unmanned rescue vehicle.

**Fig. 2.2—Launch System Characteristics — Mission Matrix for Space War Scenario (with U.S. and SU Homeland Sanctuaries)**

areas indicate missions that were in our judgment not applicable to a space war operating environment unless there were sanctuaries for space stations and resupply vehicles. During this type of conflict, the rapid replacement of all types of satellites would constitute the primary space support mission; presumably, missions requiring the orbital presence of crew would occur only in an emergency. Thus, for satellite insertion missions, unmanned launch systems are preferable to manned systems. The same observation applies to the other two space support missions—space station supply and space station crew replacement/rescue. The exception is the space rescue mission when the space station and rescue vehicles are in sanctuary. In this case, a manned launch system would be desirable.

The alternative systems are shown in Table 2.1. Because the assessment would be consistent with the availability of an SSTO, NASP-derived vehicle (i.e., 15 to 20 years into the future), the alternative systems are generic in nature. We assume the same technology base will be available for all vehicles considered. However, some design choices are more conservative than others in that the technologies required to implement the design do not push the state of the art.



**Table 2.1**  
**Candidate Launch Systems**

---

<b>Manned vehicles (on orbit)</b>
<b>Single-stage</b>
NDV
Advanced rocket-powered vehicle
<b>Multi-stage</b>
Shuttle-type vehicle
Sanger-type vehicle
Advanced rocket-powered vehicle
(recoverable first stage)
Subsonic aircraft (first stage)
<b>Unmanned vehicles</b>
<b>Single-stage</b>
HOTOL-type (see text)
Advanced rocket-powered vehicle
<b>Multi-stage</b>
Advanced rocket-powered vehicle
Sanger-type vehicle
Subsonic aircraft (first stage)

---

The NDV is assumed to be a manned, single-stage-to-orbit vehicle. For launch systems such as the Sanger type<sup>2</sup> or the subsonic aircraft, the first stage is always manned and reusable, but the second may be either manned or unmanned. The first stage of the two-stage advanced rocket-powered vehicle may be either manned or unmanned. The HOTOL-type<sup>3</sup> launch vehicle is an SSTO design that is launched from a reusable sled. It provides an unmanned capability.

Figure 2.3 presents an assessment of the ability of each candidate launch vehicle to successfully perform the six space-support missions. During peacetime, the only factors that might prevent a particular vehicle from performing a given mission are payload limitations or the absence of men-on-orbit. During a space war it is assumed that, for manned systems, prolonged time-on-orbit must be avoided. Thus, satellite launches would, preferably, be performed by unmanned systems. The NDV is assumed not to be able to launch multiple satellites because of the potential excess exposure to a hostile antisatellite (ASAT) attack. Across the spectrum of scenario contexts, NDVs perform as well as or better than any of the other launch system candidates except during a space war. During a space war, unmanned advanced rocket launchers have the best overall mission coverage.

A dilemma arises when comparing the space launch mission capabilities of NDVs and advanced rocket vehicles. Reasonable performance and cost bounds can be established for the

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<sup>2</sup>A Sanger-type vehicle is one that follows the basic design approach as the two-stage Sanger launch vehicle being developed by Germany. The first stage uses air-breathing propulsion and the second stage is a rocket-powered orbital vehicle.

<sup>3</sup>A HOTOL-type vehicle uses a similar design approach as the British HOTOL vehicle. It uses an air-breathing propulsion augmented with rockets and is launched from a sled.

	Peacetime	Space War						Conventional War						Nuclear War					
		With <sup>a</sup> sanctuaries			No sanctuaries			Without space war			With space war			Following space war					
Manned systems:																			
NDV	x x x x x x	x	x x		x	x	x x x x x	x	x			x x							
Adv. SSTO rocket	x x x x x x	x	x x		x	x	x x x x x	x	x			x x							
Shuttle-type	x x x x x x	x	x x		x	x	x x x x x	x	x										
Sanger-type	x x x x x	x	x x		x	x	x x x x x	x	x			x							
Adv. TSTO rocket	x x x x x x	x	x x		x	x	x x x x x	x	x			x x							
Subsonic first stage	x x x x x x	x	x x		x	x	x x x x x	x	x			x							
Unmanned systems:																			
HOTOL-type	x x x x	x	x x		x	x	x x x x	x	x			x x							
Adv. SSTO rocket	x x x x	x x	x x		x x	x	x x x x	x x	x			x x							
Sanger-type	x x x	x x	x		x x		x x x	x x				x x							
Adv. TSTO rocket	x x x	x x	x		x x		x x x	x x				x x							
Subsonic first stage	x x x	x x	x		x x		x x x	x x				x x							
Missions	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6	1 2 3 4 5 6				

1. Single satellite insertion

2. Multiple satellite insertion

3. Satellite inspection/retrieval

4. On-orbit satellite refurbishment/repair

5. Space station supply

6. Space station crew replacement/rescue

1. Single satellite insertion
2. Multiple satellite insertion
3. Satellite inspection/retrieval
4. On-orbit satellite refurbishment/repair
5. Space station supply
6. Space station crew replacement/rescue

<sup>a</sup> For space station and resupply vehicles.

**Fig. 2.3—Launch Vehicle Mission Assessment**

latter vehicles, but for NDVs the required technologies in such areas as propulsion, materials, and thermal protection are in development. The uncertainties associated with the eventual performance and cost of NDVs are considerably greater than those for rocket launchers, which draw upon a well-established technology base. As a consequence, at this stage of NASP/NDV development, only qualitative comparisons with alternative launch systems are feasible.

Figure 2.4 gives the results of the overall assessment of each system's ability to perform the six space missions for all four scenario contexts. It tabulates both the total mission capability (i.e., how many of the six missions can the system perform) and the number of desirable operational characteristics the system has (i.e., how well it does the missions).<sup>4</sup> For manned

<sup>4</sup>For this analysis, we assigned a value of 1 to the favorable characteristics ("yes," "probably," and "maybe") that each type of vehicle would get for each scenario; the sum of these favorable characteristics appears in Figure 2.4, where 14 is the highest possible score for the peacetime scenario context. The favorable characteristics for the space war scenario are illustrated in the matrix shown in Figure 2.2. The number of missions that a vehicle can accomplish for each scenario context is derived from Figure 2.3.

Peacetime	Space War		Conventional War		Nuclear War	
	With <sup>a</sup> sanctuaries	No sanctuaries	Without space war	With space war	Following space war	
Manned launch systems:						
NDV	6/14 <sup>b</sup>	3/10	2/8	5/18	2/8	2/8
Adv. SSTO rocket	6/9	3/6	2/4	5/11	2/4	2/4
Shuttle-type	6/5	3/2	2/0	5/4	2/0	0
Sanger-type	5/12	3/9	2/8	4/12	2/8	1/3
Adv. TSTO rocket	6/7	3/4	2/2	5/7	2/2	2/4
Subsonic first stage	6/11	3/8	2/6	4/9	2/6	1/2
Unmanned launch systems:						
HOTOL-type	3/10	3/9	2/10	3/10	2/10	1/4
Adv. SSTO rocket	4/9	4/12	3/11	4/12	3/11	2/6
Sanger-type	3/11	3/16	2/14	3/16	2/14	2/10
Adv. TSTO rocket	3/8	3/11	2/9	3/11	2/9	2/6
Subsonic first stage	3/10	3/14	2/12	3/14	2/12	2/8

Best score in manned or unmanned category for a given scenario context.
  Best score overall in manned and unmanned category for a given scenario context.

<sup>a</sup> For space station and resupply vehicles.  
<sup>b</sup> Missions favorable characteristics.

Fig. 2.4—Launch System Overall Assessment


launch systems, an NDV during peacetime can accomplish all six missions and, additionally, it has all of the desired operational characteristics that over the six missions are called for 14 times, the best of any manned or unmanned system. In a conventional war context that includes a space war, it can accomplish only two of the missions, and it has the desirable operational characteristics eight times.


From an overall perspective, the NDV scored the highest during peacetime or conventional war without space war. During either space war or conventional war that includes space war, the unmanned advanced SSTO rocket and the Sanger-type vehicles scored highest. In a nuclear war, the unmanned Sanger-type vehicle has the best combination of mission capabilities and favorable operational characteristics.

Figure 2.5 arrays the data somewhat differently, dividing the vehicles by stages rather than manning.<sup>5</sup> For single-stage vehicles, the NDV has the best ratings in three categories: peacetime, conventional war without space war, and nuclear war following space war. The SSTO rocket has the highest score for space war or conventional war with space war.

Turning to two-stage vehicles, the Sanger-type vehicle scored highest for all scenario contexts. Its high scores result from the flexibility afforded by having either a manned or unmanned upper stage. Similarly, the TSTO rocket and the subsonic aircraft launcher can have either a manned or unmanned upper stage, permitting them to perform all of the missions, but there are fewer favorable system characteristics than with the Sanger-type vehicle.

	Peacetime	Space War		Conventional War		Nuclear War
		With sanctuaries <sup>a</sup>	No sanctuaries	Without space war	With space war	Following space war
Single stage:						
NDV (manned)	100:14 <sup>b</sup>	75:10	66:8	100:18	66:8	100:8
Adv. SSTO rocket (manned & unmanned)	100:12	100:13	100:11	100:14	100:11	100:6
HOTOL-type (unmanned)	50:10	75:9	66:10	60:10	66:10	50:4
Hybrid stage:						
Shuttle-type (manned)	100:5	75:2	66:0	100:4	66:0	0:0
Two stage:						
Sanger-type (manned & unmanned)	100:17	100:19	100:16	100:19	100:16	100:10
Adv. TSTO rocket (manned & unmanned)	100:12	100:13	100:10	100:14	100:10	100:6
Subsonic first stage (manned & unmanned)	100:13	100:16	100:13	100:16	100:13	100:3

 Best score in single-stage or two-stage categories for a given scenario context

 Best score overall for both single-stage and two-stage categories for a given scenario context.

<sup>a</sup> For space station and resupply vehicles

<sup>b</sup> Percent of possible missions favorable system characteristics

Fig. 2.5—Composite Launch System Overall Assessment

<sup>5</sup>In Figure 2.5, we have used the combination of percentage of possible missions and favorable system characteristics as qualitative measures of how well a launch system performs. This allows the comparison of single, hybrid, and two-stage vehicles that are manned and unmanned, when the maximum number of total missions are different for some scenario contexts—peacetime, space war, and conventional.

However, of both single- and two-stage vehicles, the Sanger-type vehicle has the highest score for all levels of conflict. This observation could also apply to the NDV if an unmanned orbital payload package was developed that could accomplish the unmanned space support missions without exposing the manned launcher to ASAT attacks. This is not an option in the current NASP program.

Summarizing the findings of Figures 2.4 and 2.5, we observe:

- Compared to other manned launch vehicles, NDVs score higher for all scenario contexts.
- Compared to the best of the unmanned vehicles, NDVs score higher in peacetime but lower in all scenarios involving space conflict.
- Two-stage vehicles with a manned or unmanned upper stage offer the most robust options across the spectrum of scenario contexts from peace to nuclear war.

The above discussion represents the approach taken for one of the four broad mission areas in which the NDV might be useful. We followed a similar process for the other three missions. The general conclusions of that extensive analysis appear below.

## RESULTS OF SINGLE-MISSION ANALYSIS

The results of our scoring system suggest that the NDV can perform all of the generic missions, but so can many of the alternative systems. No single mission stood out as clearly justifying the NDV in and of itself. The NDVs have competitors for every mission, and these competitors do not generally have the same degree of complexity as an NDV. But the issue does not end there, because the ability of the NDV to perform *all* of the generic missions indicates that it may have a stronger role in a composite mission.

## COMPOSITE-MISSION ANALYSIS

While the NDV can, in principle, do all of the generic missions and do many of them well, it does not appear to have any overwhelming advantage over other ways of accomplishing the same mission. But the NDV does appear to have a substantially broader range of application than any of its competitors. Therefore, it is possible that the composite role of the NDV provides its best rationale, and that a small fleet of them might well serve the National Command Authority.

However, it must be understood that a small fleet of NDVs would appear adequate for the composite applications role and that an NDV capability would require substantial and difficult RDT&E on many subsystems. This RDT&E would be in addition to existing challenges associated with the basic X-30 program.

The composite mission capability proves especially attractive in light of today's political and military realities. Although alternatives to the NDV exist for each mission area, their feasibility in today's climate of reduced budgets is less sure. Should some of the alternative programs fall victim to budget cutting, then the multiple capabilities of an NDV appear more attractive.

## IMPLICATIONS OF MISSION ANALYSES

RAND studies have thus far found no clearly compelling set of reasons for exclusive concentration on single-stage-to-orbit versions of an NDV. The Space Council's phrasing, in its 1989 review, "with the ultimate goal of single-stage-to-orbit," is easily interpretable as being the basis for exclusive focus on that goal, and is therefore in some sense ambiguous, it seems to us. RAND's opinion is that consideration of a two-stage-to-orbit vehicle would be within the spirit of the Space Council's NASP program guidance. This interpretation is one of several possible and we do not claim otherwise. We explore this situation in more detail in Section 6, particularly where we discuss a proposed modified RDT&E program and delineate its compatibility with the general outlines of the Space Council guidance.

Along with emphasis on the "composite utility" multi-mission potential application roles for an NDV, RAND continues to believe that a comprehensive review of appropriate aspects—RDT&E policy choices, technology base and technology derivations, estimates of costs and schedules, and the like—of comparisons among single and multiple stages, alternative and hybrid propulsion cycles, and a balanced strategy of risk moderation and expanded technology options is warranted in Phase II of the NASP program before any Phase III decisions are made.

### 3. THE TECHNOLOGICAL CHALLENGE

Although RAND did not undertake a technological review of the NASP project, its charter for an integrated system overview included consideration of the enabling technologies associated with the project. Certainly, formidable scientific challenges surround this project. Each technology is demanding in itself. The requirement to integrate a number of them in a manner unparalleled in any other civilian or military system compounds the problem. A deficiency in any one promises to hold the entire project hostage.

#### INDEPENDENT REVIEWS

RAND did not address the technology issue in depth partly because both the Defense Science Board (DSB) and the Air Force Studies Board (AFSB) reviewed the technologies associated with the project in 1987 and 1988, respectively. The DSB observed that the exploration of hypersonic flight and design of X-30-like vehicles to accomplish that exploration involves six critical technologies: aerodynamics, supersonic mixing and fuel-air combustion, high-temperature materials, cooled structures, control systems, and computational fluid dynamics (CFD). The DSB reviewed these technologies in depth and concluded that the then-extant schedule for technology maturation (scheduled for September 1990) was unrealistic.

The AFSB report reviewed propulsion-airframe integration; propulsion systems; aerodynamics; control, guidance, and information systems; materials structures and cooling; the role of CFD; and test requirements. Both the DSB and AFSB groups had essential unanimity as to where the technological challenges were.

Both groups focused on the technologies associated with SSTO. However, it does not necessarily follow that such a focus should be understood as excluding other methods of realizing the program objectives.

Of course, views on any complex project diverge, and the NASP project does not differ in that regard. One school of thought holds that each technology must first mature before moving to the integrating state. The opposing view pushes for early integration, arguing that a failure to show quick system progress will erode commitment to the project and, ultimately, doom it. Both views have merit.

#### RAND REVIEW

RAND looked closely at two critical technologies that bulk large in the success of this project: computational fluid dynamics and propulsion. RAND drew on the work of the Defense Science Board and the Air Force Studies Board as well as its own considerable experience in these areas.<sup>1</sup>

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<sup>1</sup>In addition to having direct access to the NASP technology and vehicle contractors during the study, RAND also frequently updated its informational data base through early 1991 and periodically thereafter through the NASP Technology Review symposiums. In the Fall of 1989, the NASP JPO held at RAND an intensive three-day technology symposium at which all the primary NASP contractor teams presented major technology reviews. RAND also presented the study findings on NASP and their implications for Air Force hypersonics interests to the USAF

## COMPUTATIONAL FLUID DYNAMICS

The state of CFD has a critical effect on the NASP project. Over the years, a number of facilities suitable for testing components of hypersonic vehicles have degraded or disappeared.

CFD techniques can partially offset these losses if they can promise high reliability. Confidence in the predictions and the ability to validate predictions by experiment become central to the success of the project.

**DSB Review of CFD.** The DSB reviewed the NASP project in 1987 and rendered its report in 1988. The DSB took a prudent and cautionary stance on CFD. It noted the significant advances made in CFD while indicating that much remained to be done, particularly with respect to code validation and calibration. The work in this area is especially critical because small variations in some large numbers spell the difference between success and failure, such as achieving orbit or not.

**AFSB Review of CFD.** The AFSB effort took place in 1987 and 1988; the report was published in 1989. The AFSB identified issues similar to those of the DSB. It voiced reservation whether CFD could compute the phenomena with sufficient accuracy above certain speeds. The limitations noted are not inherent to CFD itself but rather stem from the current state of computer development, which forces a time-averaged use of Navier-Stokes equations. Better computational methods exist, but they will have to await the development of better computers to take advantage of them.

Shortly after the publication of the AFSB report, the NASP JPO prepared a status report for the Space Council. Its report presented a sanguine view of the power of CFD techniques then available. Although some progress in CFD had occurred since the DSB and AFSB did their work, the question remained whether it was sufficient to render irrelevant the concerns of the two scientific bodies. The question is critical, and the absence or sparseness of test facilities makes it even more so.

**RAND Review of CFD.** RAND reviewed the status of CFD in some detail,<sup>2</sup> leading to the following conclusions:

- CFD simulations are gradually replacing or supplementing wind tunnel experiments for a class of well-understood aerodynamic configurations and applications.
- In terms of current modeling, the accuracy of CFD compared to experimental results in the lower hypersonic range is about six to seven percent. This level of accuracy falls below what would be required for high confidence in the results, particularly for certain flow situations that are likely to be important for NASP, and when limited data exist to guide computational analyses.
- Given the absence of substantial validating data for the CFD codes at higher hypersonic speeds, the errors will likely be much larger. This area needs a great deal of research.

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Science Advisory Board's panel on Hypersonics in January 1991. The SAB offered no negative comments on RAND's technology assessment.

<sup>2</sup>See S. K. Liu, *Numerical Simulation of Hypersonic Aerodynamics and the Computational Needs for the Design of an Aerospace Plane*, RAND, N-3253-AF, 1992.



- Based on past trends, the expected speed of the fastest computer will still fall short of what is needed to raise CFD to a high-confidence, self-contained design tool for hypersonic aircraft applications around the year 2000.

In short, the RAND review turned up little evidence that the concerns expressed by the DSB and AFSB have been resolved. These remain open questions not likely to see resolution by the spring of 1993. In the absence of a concerted program to obtain appropriate experimental data points to better anchor CFD analyses, the year 2000 seems more likely for a reasonably effective first-principles computational approach to NASP design.

There are, of course, counter arguments to this position. One can argue that research programs at first flight do not necessarily need to incorporate fully mature technologies. Another argument holds that advanced aircraft, like the X-15 and SR-71, explored flight envelopes with less mature technologies by gradually expanding flight parameters to reach objectives.

With respect to the first argument, we note that the NASP is not just any research program. It has extremely high visibility, and it is expensive. It needs reasonable assurance that it can meet its objectives. The X-15 and SR-71 make poor analogues to the NASP. Both aircraft were much closer to a solid base of experimental data, which made their advances far less risky than the NASP.

In sum, our judgment is that an active and intensive program of experimental data anchoring should go hand-in-hand with CFD capability growth in the next few years. RAND, therefore, has proposed a program for hypersonic RDT&E that bolsters the experimental plan to make the joint effort of well-defined experiments and CFD capability development more productive in the near term and less risky than an excessive or unbalanced reliance on CFD.

## PROPULSION

**DSB Propulsion Review.** The DSB review of propulsion noted, among other things, that current test facilities could not provide data much more above Mach 8 and that full-scale testing would fall below that. It considered three propulsion phases: low speed, RAMJET, and SCRAMJET. It raised questions about each phase. The problems of integrating the low-speed propulsion component with the higher-speed systems were viewed as significant. The second phase, conversion to supersonic, introduces a number of difficult design and development problems. The final, high-speed phase adds the problems of shock, cooling, and the combustion process itself.

The DSB review further noted that desired facilities for testing the high-speed engine component did not exist and were unlikely to be built. The alternative, restricted simulations of individual component behaviors, would still require demanding new test facilities.

**AFSB Propulsion Review.** The AFSB review covered much of the same ground and reached many similar conclusions. It also emphasized the notion of a separate auxiliary rocket propulsion system to perform essential functions during the flight test program, arguing that a separate propulsion system was needed during the flight test to allow a range of

conditions under which to test the main engines. It viewed the added complexity of a separate engine and set of controls as acceptable.

Progress since the reviews by the two boards has included advances in CFD, experiments, and improved facilities. Still, many of the questions raised remain open. The JPO flight test philosophy remains one of gradual expansion of the flight envelope using the basic NASP engine. As problems crop up, the JPO intends to resolve them by working on the flight vehicle itself during the test program. This approach raises two questions. First is the practical one of convincing decisionmakers to proceed with a manned flight program employing incompletely tested critical elements. The highly visible nature of the NASP program will make this decision difficult. The second question relates to managing the addition of a series of possibly complex modifications to an already complex, integrated airframe/propulsion design.

**RAND Propulsion Review.** Unlike the DSB and AFSB reviews, RAND's effort extended to alternatives to the SSTD option. It included the old NASP program, which considerably exceeded the technical breadth in propulsion of today's program. That program proposed a complementary ground- and flight-testing program. But some of the facilities were never built, and others have fallen into disuse. Some remain, and plans exist to develop others.

Full or subscale flight tests to complement the ground test remain a matter for argument. The notion of a subscale flight vehicle to produce data anchoring points for CFD in areas beyond the capability of ground test facilities has a number of points of interest. Certainly, successful precedent exists.

Clearly, much more remains to be done in integrating unmanned and subscale testing into the NASP program. The next stage of subscale flight testing could involve development of a new carrier aircraft launch vehicle. A number of rocket-power configurations pertinent to carrier aircraft have been studied. The earlier NASP program featured aircraft of this sort. It seems reasonable to conclude that a workhorse carrier vehicle with a limited Mach range would not pose a significant development risk and would allow testing of a range of engine types.

## SUMMARY

In the two major technology areas, CFD and propulsion, there are continuing uncertainties for NASP vehicle design and full evaluation. The time for resolving these uncertainties looks considerably longer than the one or two years sometimes mentioned. Progress in critical areas has not resolved these problems. The sparseness or absence of hard-data anchoring points and the limits of automated computational power restrict the accuracy of determining vehicle performance and defining vehicle characteristics to ensure that performance.

One approach might be to set aside these uncertainties and seek to modify the vehicle design in the process of extending the full-scale vehicle flight envelope in the test program. Another, less precarious approach is to conduct a concerted program of flight experimentation along with CFD analysis using experimental anchoring points, and thus develop a high-confidence design guide for structuring the full-scale vehicle program. RAND believes that developing and reviewing arguments for either approach ought to figure prominently in the Space Council's forthcoming evaluation on when to proceed with acceptable levels of risk in design, fabrication and flight of NASP vehicles.

## **4. COST CONSIDERATIONS**

The usefulness of NDVs will depend as much on their economics as on their utility. If they are relatively cheap, they will find a number of roles and missions, but if they are expensive they will have a hard time finding a niche. Our analysis of the costs surrounding the NDV program leads to two conclusions. First, substantiating that NDVs will significantly lower the cost of space launches is extremely difficult. Second, decoupling the X-30 research vehicle development schedule and the NDV RDT&E program will lead to lower costs than will a concurrent acquisition strategy.

### **COST-ESTIMATING RELATIONS (CER) ARE UNCERTAIN**

As both the DSB and the AFSB noted, major technological progress has been made in this program, but advances in areas such as materials are still needed. The consequences of the uncertainties surrounding the program are likely to manifest themselves in design changes as the program progresses. In fact, changes to the designs thus far have substantially increased the takeoff weight for the same orbital payload, a not unusual occurrence in such an undertaking.

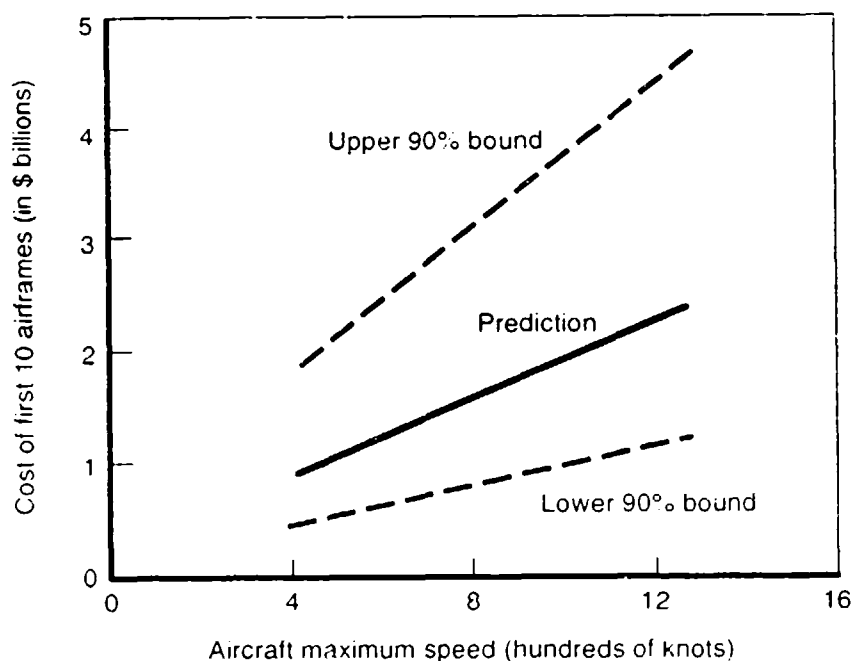
These factors increase the uncertainty surrounding cost estimates. Most cost-estimating procedures assume a constant technology, which is certainly not the case with the NDV. Analogs can provide approximate figures, but the uncertainty of the fundamental design aspects further exacerbates the difficulty of deriving reasonable estimates. Applying known aircraft cost-estimating experience to the very different NDV injects further ambiguity into the process.

The X-30 could provide a basis for estimation, although the NDV will differ from the experimental vehicle. Whatever roles the NDV assumes will require capabilities and characteristics not present in the X-30. These differences will require new subsystems, which inevitably lead to increased vehicle-to-payload weight ratios.

### **A COST-ESTIMATE EXAMPLE**

The considerations outlined above make a traditional cost estimate difficult. However, we can determine if the NDV offers savings in placing payloads in orbit by making some conservative assumptions and applying cost-estimating relationships RAND has developed for more traditional airframes.

The RAND cost model allows derivation of an airframe cost as a function of aircraft maximum speed. Applying the methodology against an aircraft designed to fly at 800 knots yields an airframe cost of \$1.5 billion for the first 10 airframes (Figure 4.1). Using an upper and lower 90 percent boundary produces an upper cost of \$3.0 billion and a lower one of \$.75 billion, or an uncertainty range of 4:1.



- R-3255-AF, post-1960 aircraft
- Average empty weight of aircraft = 48,000 lb
- Does not include avionics, propulsion, RDT&E, or O&S

SOURCES: R. W. Hess and H. P. Romanoff, *Aircraft Airframe Cost Estimating Relationships: All Mission Types*.

RAND, N-2283 1-AF, December 1987, and R. W. Hess and H. P. Romanoff, *Aircraft Airframe Cost Estimating Relationships*, RAND, R-3255-AF, December 1987.

**Fig. 4.1—Prediction Uncertainty Large for Current-Technology Airframes**

The aircraft in this estimate have designs using well-documented conventional materials that have both production capacity and vigorous commercial markets. Applying this formula to the far more exotic NDV design should produce very conservative estimates because the new types of materials used in NDV designs do not have a well-documented base of physical properties or manufacturing techniques. In addition, the NDV airframe will be cooled using the hydrogen propellant, producing a more complicated airframe design. Applying this CER to the NDV requires a manifold increase in speed, which complicates the extrapolation rationale. This extrapolation, which is well beyond the speed regime associated with the current aircraft in the database, is not only risky but increases the uncertainty of the resulting cost estimates.

With these reservations in mind we selected what we believe is a conservative approach and applied the 90 percent upper and lower bounds shown in Figure 4.1 to the NDV and developed three cases for comparison: lower, mean, and upper. The cost estimates for the mean projection for the first 10 NDV airframes only are \$13.2 billion, \$4 billion for the lower case

and \$50 billion for the upper. Using the very optimistic assumptions in Table 4.1 below, we then calculated the airframe costs of placing payloads in orbit.

Figure 4.2 depicts for the *mean projection case* the basic inputs of number of flights per year, payload lifted per year, and number of NDVs. It appears as a carpet plot,<sup>1</sup> which is a technique for allowing interpolation among three variables. In this figure, the ratio between airframe cost and payload capability (\$/lb in orbit) appears as a function of the number of NDVs procured and the total number of flights per year.

If we assume a fleet size of 10 NDVs and 40 flights per year, each aircraft flies four times per year. The annual payload lifted to orbit is 800,000 lb or 80 percent of the expected demand in 2010.<sup>2</sup> If this rate continues for 20 years, the NDV fleet will deliver 16 million pounds to LEO. Thus, Figure 4.2 shows a launch cost of \$825 per pound resulting from the procurement of only the airframes for 10 NDVs.

Figure 4.2 further shows that launch costs can vary from a few hundred dollars per pound to several thousand dollars. If we derive some reasonable boundaries, we can further refine the estimate. Several studies have estimated the annual launch demand in four cases: DoD enhanced, civilian enhanced, nominal, and constrained. The first case assumes full SDI deployment; Mars and lunar missions drive the second. The annual demand ranges from 500,000 lb to 5 million. For this study, we chose the nominal case of 1 million pounds annually, based on the results of R-3820-AF.

The number of NDV flights without major refurbishment is another reasonable predictable boundary condition. With, for example, a 50-flight limit per airframe, a fleet of 20 NDVs could fly 50 flights a year for 20 years. Contractors estimate a 100-flight life for the NDV before refurbishment.

Table 4.2 reflects the minimum launch costs for the various NDV fleets after applying the two boundary conditions to three different cost prediction cases: lower 90 percent boundary, mean projection, and upper 90 percent boundary.

**Table 4.1**

**Conditions for Analysis of the Airframe Cost Component of NDV Launch Costs**

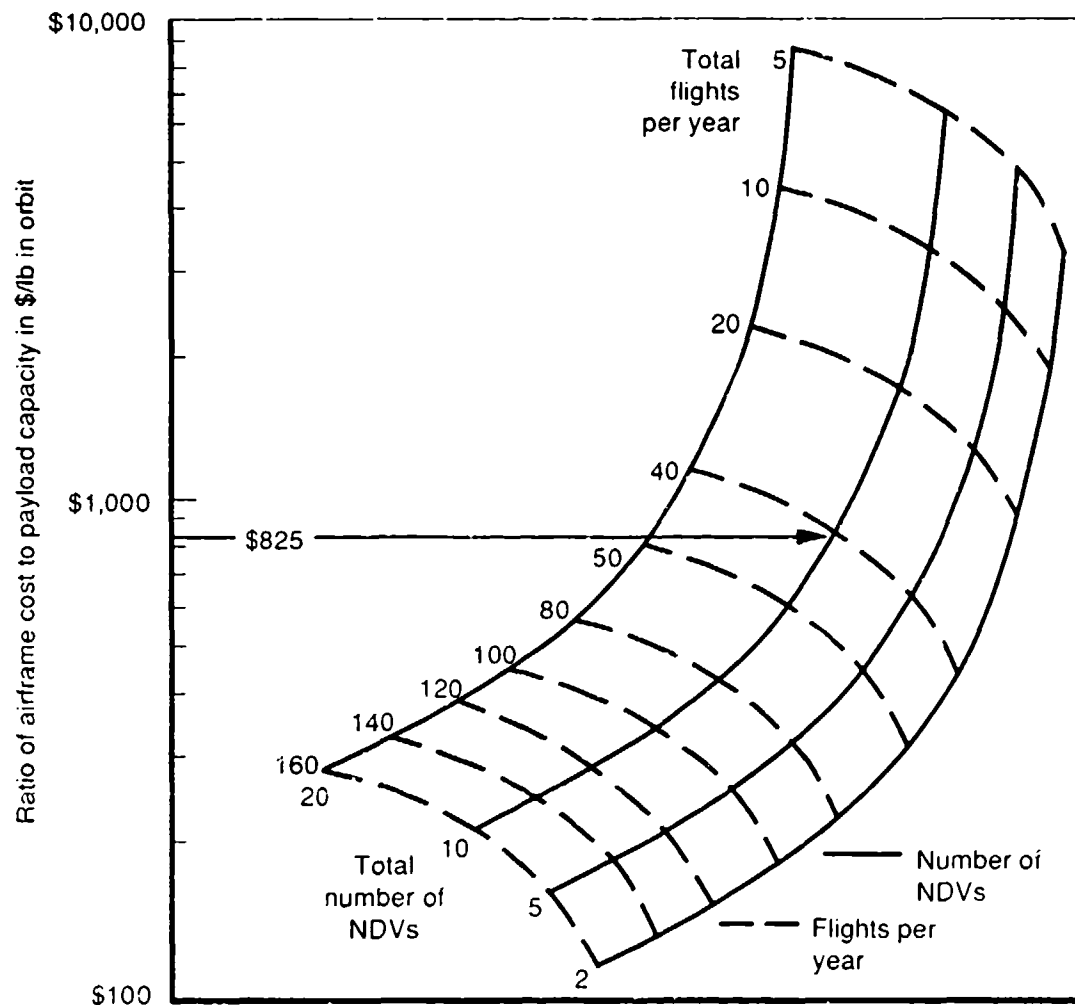
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NDV advanced technology airframe cost is scalable with current airframe cost-estimating relationship.
NDVs always carry maximum payload.
NDVs can handle 100 percent of the projected military and civilian space launch demand in 2010.
NDVs go only to low earth orbit (LEO) (i.e., no second stage)
The costs of the following items are assumed to be zero:
RDT&E
Operations, maintenance, and support
Money
Other systems (e.g., propulsion, avionics)
Initial spares

---

<sup>1</sup>A carpet plot is a technique for presenting three variables in such a way to allow interpolation between the variables. As one moves along a curve of constant value for one of the variables, the values of the second variable are separated by equal distances along the horizontal scale. As a consequence, one can interpolate between variables by using the horizontal scale.

<sup>2</sup>Scott Pace, *U.S. Access to Space: Launch Vehicle Choices for 1990-2010*, RAND, R-3820-AF, March 1990.



**Mean projection case assumptions:**

- NDV airframe cost is \$13.2 billion for 10 vehicles
- Payload = 20,000 lb per flight
- Vehicle has 20-year flight life
- Airframe cost does not include R&D, O&S, avionics, or propulsion
- Based on RAND's airframe cost-estimating relationships (R-3255-AF)

**Fig. 4.2—Estimated Contribution of Airframe Cost to NDV Launch Costs per Pound of Payload**

The results of this airframe cost projection indicate that costs can vary substantially, from \$200 to \$7600 per pound, depending upon the size of the fleet and the cost-estimate boundary case. This range suggests that claims reductions in the cost of space launch should be received with a great deal of caution.

**Table 4.2**  
**Minimum NDV Launch Cost (Airframe Only)**  
 (cost is \$/lb of payload)

Case	NDV Fleet Size			
	2	5	10	20
<b>Lower 90% boundary</b> (\$4 billion for first 10 airframes)				
100 flights/airframe	\$600	\$300	\$200	
1 million lb/yr launch			\$200	\$242
<b>Mean projection</b> (\$13.2 billion for first 10 airframes)				
100 flights/airframe	\$2000	\$1000	\$660	
1 million lb/yr launch			\$660	\$800
<b>Upper 90% boundary</b> (\$50 billion for first 10 airframes)				
100 flights/airframe	\$7600	\$3800	\$2500	
1 million lb/yr launch			\$2500	\$3040

An optimistic set of cost and operating assumptions (see Table 4.1) provides a floor for the expected launch costs that such a vehicle might achieve. Table 4.3 depicts the summary minimum costs of the three cases for an NDV fleet of ten vehicles.

Cost estimates should take other items into consideration. The above figures reflect only airframe acquisition costs and do not consider subsystem and RDT&E costs. The figures also reflect the assumption that the NDV fleet will handle the entire launch demand in 2010. Each of these items increases the expected cost of placing payloads in orbit.

Experience with other aircraft allows an estimate of the distribution of costs between the airframe and other systems. The airframe for the F-15, for example, represents only 53 percent of the total cost (only 38 percent for the F-16). Arguments can be made that the sophisticated nature of the NDV airframe should justify a higher percentage of the total cost. However, the propulsion, armaments, and avionics subsystems will be equally sophisticated. Simply assuming that the airframe represents 50 percent of the cost doubles the launch costs.

If the NDV fleet does not capture the entire launch demand, costs will increase further. Given the experience with the Challenger disaster and the emphasis the nation is placing on

**Table 4.3**  
**Minimum NDV Launch Costs for**  
**Ten-Vehicle Fleet**  
 (cost is \$/lb based on airframe  
 cost only)

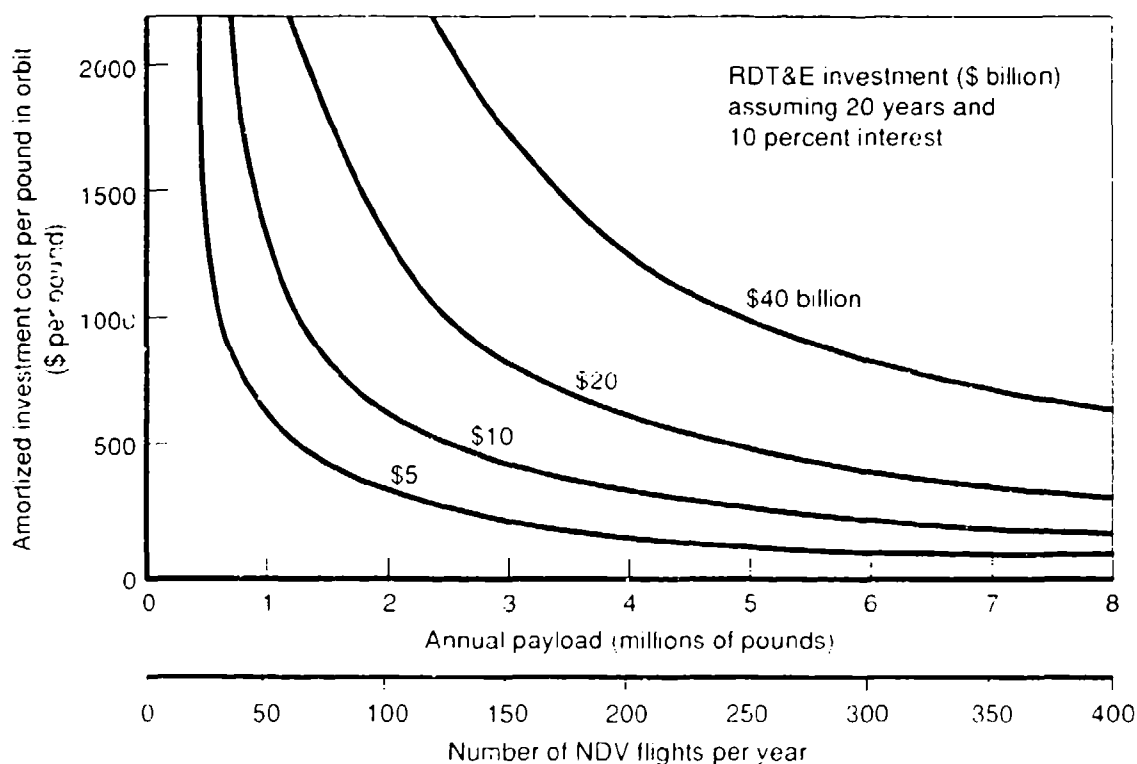
Case	Airframe Cost Only
Lower 90%	\$200
Mean	\$660
Upper 90%	\$2500

assured access to space, it does not seem likely that the United States will confine its entire launch capability to a single type of vehicle. In addition, Air Force studies have shown that payload weight and size constraints would prevent the NDV from carrying all of the projected payloads in the mission model. Assuming that the NDV fleet delivers one-half of the demand and produces the results reflected in Table 4.4.

The status of the NASP program is such that years of investment in RDT&E will be required to bring an NDV to full-scale projection. Figure 4.3 shows the amortized investment cost per pound in orbit as a function of the total investment in billions of dollars and the annual pay-

**Table 4.4**  
**Minimum NDV Launch Costs for Ten-Vehicle Fleet**  
(cost is \$/lb based on airframe cost only)

Case	Airframe Cost Only	Other Subsystems	50% Launch Demand
Lower 90%	\$200	+\$200	+\$400
Mean	\$660	+\$660	+\$1320
Upper 90%	\$2500	+\$2500	+\$5000



**Fig. 4.3—Amortizing Investment Costs**



load launched into orbit. For the projected annual payload of one million pounds (i.e., 50 NDV flights), the resulting launch cost increment for investment would be approximately \$1700 per pound if the investment is about \$13 billion. As seen from Figure 4.3, the amortized investment cost per pound increases dramatically for small changes in investment when the annual payload is one million pounds; it would exceed \$2000 if the investment reaches \$18 billion. Two other factors will lead to higher values: first, as mentioned earlier, it is not expected that the NDV will be used for all the annual payload demands and, second, interest will be accrued for the procurement of the vehicles as well as for RDT&E. Very conservatively, we have used \$1700 per pound for the amortized investment increment in Table 4.5.

Table 4.5 summarizes the effect of the cost increments. It can be seen that the minimum launch costs have increased from the \$200/lb–\$2500/lb range for airframes only to the \$2500/lb–\$11,700/lb range when one adds the other cost elements.

Launch costs, however, contribute only a portion of the costs of space operations. In fact, numerous past studies have indicated that payload acquisition costs far exceed launch costs.

A recent study by the Office of Technology Assessment compared launch costs with those of buses and payloads:<sup>3</sup>

Because of the high cost of spacecraft, a dramatic reduction in launch cost alone will not substantially lower spacecraft program costs. Although launching a pound of payload to LEO currently costs about \$3,000, procuring that pound of payload typically costs much more. For example, representative U.S. spacecraft buses of types first launched between 1963 and 1978 cost between \$130,000 and \$520,000 per pound dry, including amortized program overhead costs. Procurement of the mission payloads carried on those buses cost about 50 percent more—about \$200,000 to \$800,000 per pound. Reducing launch costs from \$3,000 to \$300 per pound of payload, a goal of the Advanced Launch System program, would reduce the total cost—by less than 2 percent.

Such considerations suggest it is far more productive to worry about reducing payload costs rather than launch costs. Arguments for an NDV that reduces costs to, say, \$100 per pound seem curiously misdirected.

**Table 4.5**  
**Minimum NDV Launch Costs for Ten-Vehicle Fleet**  
(cost is \$/lb based on airframe cost only)

Case	Airframe Cost Only	Other Subsystems	50% Launch Demand	Investment	Total
Lower 90%	\$200	+\$200	+\$400	+\$1700	\$2500
Mean	\$660	+\$660	+\$1320	+\$1700	\$4240
Upper 90%	\$2500	+\$2500	+\$5000	+\$1700	\$11700

<sup>3</sup>U.S. Congress, Office of Technology Assessment, *Affordable Spacecraft: Design and Launch Alternatives—Background Paper*, OTA-BP-ISC-601, U.S. Government Printing Office, Washington, D.C., January 1990.

## **SUMMARY OF COST CONSIDERATIONS**

The above calculations, which do not include O&S of the NDV fleet, indicate that costs of space launch can vary dramatically. Proceeding from some very conservative assumptions, the average case (mean projection) approaches today's costs.<sup>4</sup> We think it far more likely the costs will approximate those of the upper 90 percent case. We thus regard claims of substantial reductions in space launch costs as suspect.

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<sup>4</sup>See Pace, R-3820-AF, March 1990.

## 5. ENVIRONMENTAL ISSUES

The debates over the SST in the late 1960s addressed many topics, with environmental concerns figuring prominently. These concerns questioned sonic booms and possible effects on stratospheric ozone. Environmental interests have, if anything, sharpened over the years, and the NASP program must take them into account. Worry over the environment did not single-handedly bring down the SST program, but it had a powerful influence. In today's climate, such issues, if not adequately addressed, might be sufficient to delay the project significantly if not terminate it altogether. If, on the other hand, NASP displaces other more environment-defiling systems, then it could be a net gain.

The SST program contains some useful precedents. The primary environmental concerns raised during that debate centered on ozone depletion, sonic booms, and engine noise. These issues will be raised again as the NASP program proceeds, so prudence dictates their early consideration.

### OZONE DEPLETION

In 1971, the Massachusetts Institute of Technology (MIT) sponsored an investigation of the possible impact of water vapor, CO, and NO<sub>x</sub> on stratospheric ozone.<sup>1</sup> It was concluded at that time that CO and NO<sub>x</sub> are much less significant than water vapor and could be neglected. It was pointed out that the water vapor from the SST would decrease the stratospheric ozone.

Later, other scientists postulated that nitrogen oxide (NO<sub>x</sub>) could destroy ozone. The NASP produces both water vapor and, during the reentry phase, nitrogen oxides. That an effect on the ozone occurs is not at issue. The question is how large the effect is.

The calculations are extremely complex, and, in fact, the full set of equations has not been fully characterized or solved.<sup>2</sup> With conservative assumptions about NASP characteristics, a typical aerospace vehicle would produce 860,000 kg of water vapor over its flight path. NASP also produces NO<sub>x</sub>, both during stratospheric cruise and reentry.

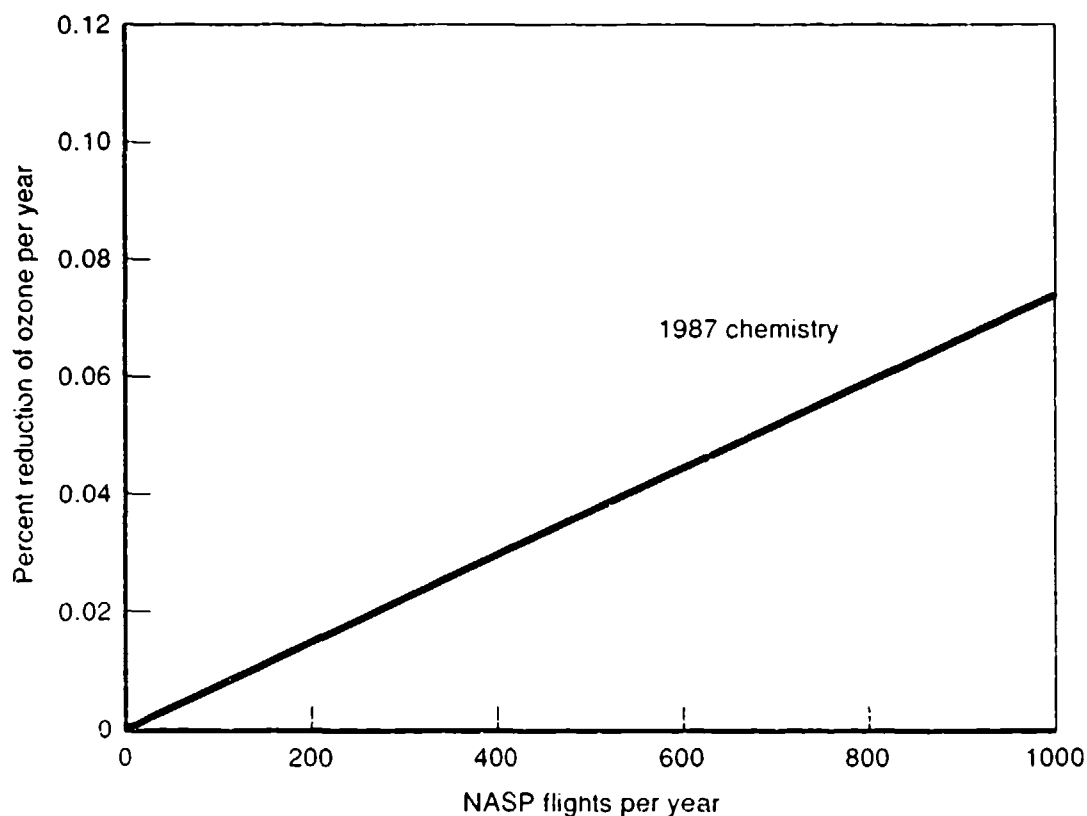
Two hundred NASP flights per year<sup>3</sup> would emit approximately 1900 metric tons of nitrogen oxide, which equates to an ozone reduction of about 0.0134 percent per year. Figure 5.1 depicts the effects.

The estimation of the direct effects of water vapor on ozone levels requires a photochemical equilibrium model of ozone in an atmospheric column that includes water vapor and hydro-

<sup>1</sup>Massachusetts Institute of Technology, *Man's Impact on the Global Environment*, Report of the Study of Critical Environmental Problems (SCEP), MIT Press, Cambridge, Massachusetts, 1970.

<sup>2</sup>See S. K. Liu, *Aerospace Flights and Stratospheric Ozone: Review and Preliminary Assessment of National Aerospace Plane Operations*, RAND, N-3464-AF, 1992.

<sup>3</sup>As discussed in Section 4, the likely number of NASP flights per year for the space launch mission is about 50 or less; the 200 flights used here illustrate the conclusion that even for an optimistically high flight rate, the ozone reduction is very low.



**Fig. 5.1—Estimated Ozone Reduction from NO<sub>x</sub> Emission for Various Levels of NASP Operations**

gen. The summary results in Figure 5.2 relate water vapor increases from NASP operations to ozone decreases and rises in surface temperatures. They show an annual ozone decrease of about 0.0020 percent annually.

A linear addition of the two predominant effects of 200 annual NASP flights yields a net ozone decrease of about 0.016 percent annually ( $0.0134 + 0.0020$ ). Although any change may be viewed as detrimental, this falls far below other man-caused effects. In addition, the expected annual number of operational NDV flights is expected to be 50 or fewer, reducing the ozone impact.

## SONIC BOOM

Sonic booms should be addressed in the design of the aircraft. International regulations and federal laws ban regular supersonic flights if the vehicle's ground level overpressures exceed certain limits. International regulations require overpressures of under 2 lb/sq ft during acceleration and below 1.5 lb/sq ft during cruising. Further, what the general population might view as acceptable might fall below these limits. Overpressure constraints may severely limit the NASP's gross vehicular weight, its mode of operation, or its configuration.

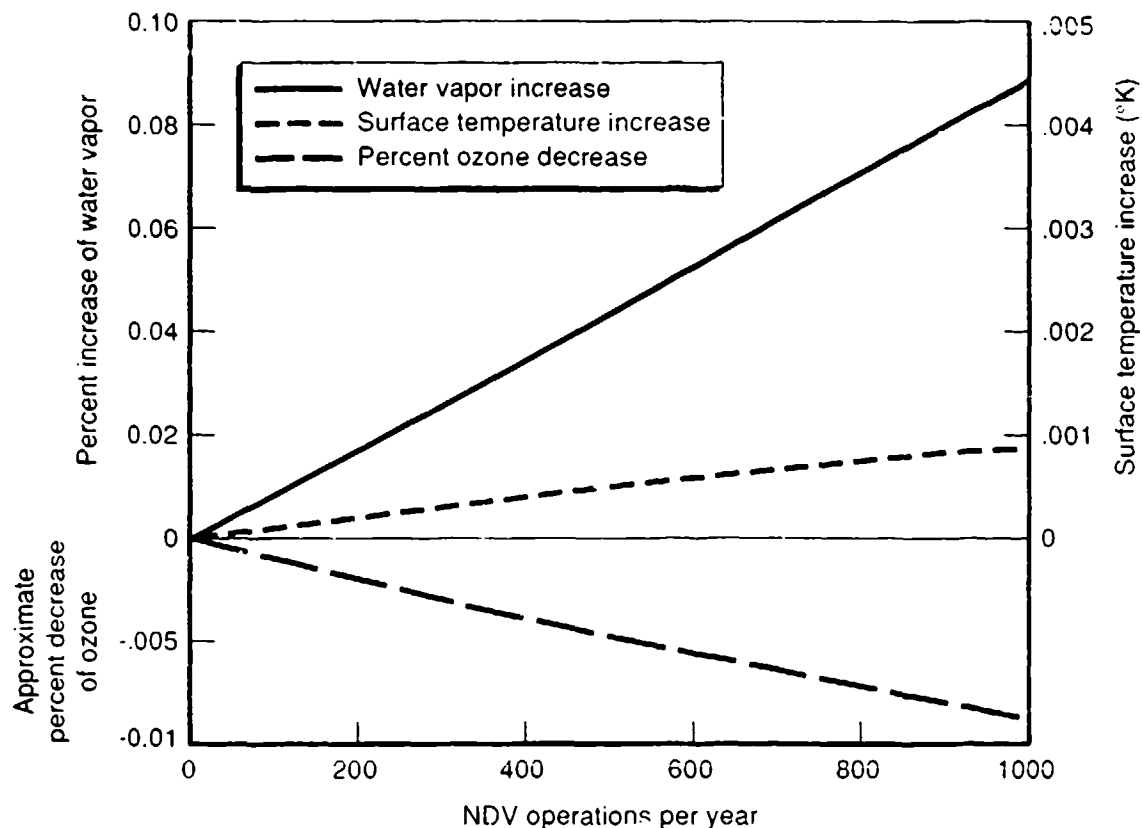


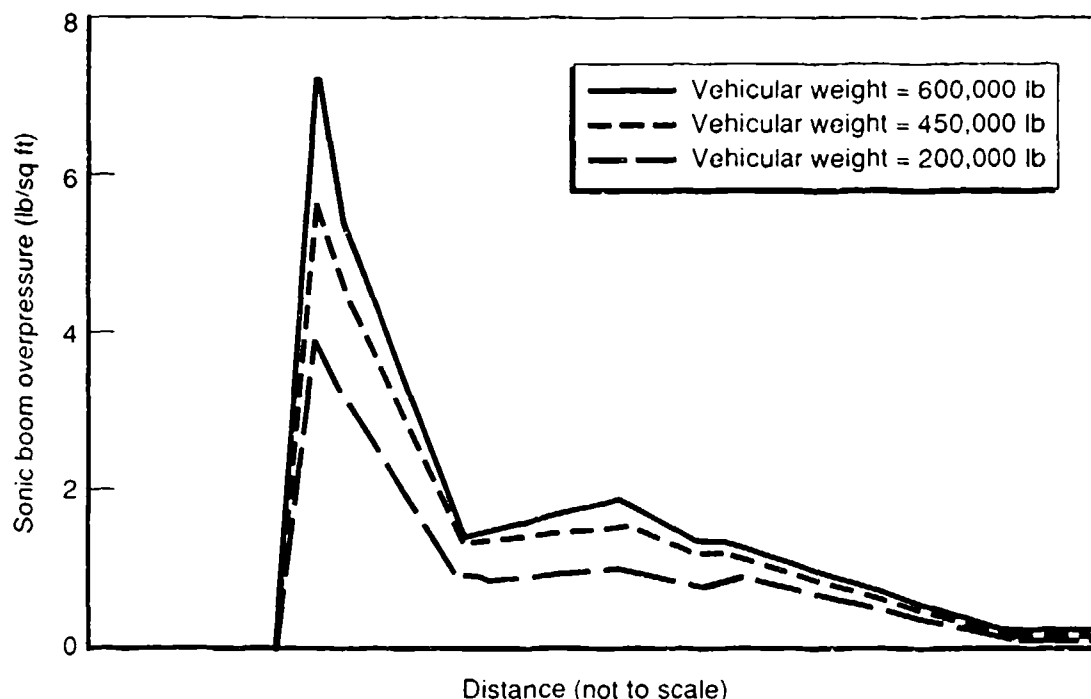
Fig. 5.2—Effect of Increased Water Vapor on Ozone

As they are for ozone depletions, calculations involving sonic booms are complex. Further, sonic boom research over the past 15 years has been fairly inactive. Still, experience with other aircraft such as the SR-71 and wind tunnel experiments allow predictive calculations if we specify aircraft characteristics and operational parameters. However, the classic sonic boom theory has not yet been modified for hypersonic flight of NASP-like vehicles.

Figure 5.3 shows predicted overpressures as a NASP accelerates to orbital speed, with typical flight paths to orbit and initial vehicle weights of 200,000, 450,000 and 600,000 lb. The possibility of focusing effects or inhomogeneities that could lead to super booms was not considered.

A corresponding set of calculations can be made for constant speed at various altitudes and initial gross vehicle weights. Some of these results appear in Figure 5.4.

The estimated ground-level overpressure intensities for the various aircraft postulated indicate that overpressures range from less than 1 lb/sq ft to almost 8 lb/sq ft. Further, these effects would spread across the ground in widths ranging from a few miles to one hundred miles.



**Fig. 5.3—Ground-Level Signature Strength Under Various NASP Fly-Weight Scenarios**

Our research shows that 1.5 to 2.0 lb/sq ft mark the highest acceptable level for sonic boom overpressures, with indications that levels below 0.5 to 0.8 lb/sq ft will become routine requirements for overland supersonic flights.

These considerations suggest that flight testing of a full-scale NASP and the progressive flight envelope development contemplated warrant careful public preparation for sonic boom incidents and a campaign to anticipate and allay public concern over deleterious environmental effects. They also suggest that a serious attempt be made to develop a proper sonic boom theory for a hypersonic airplane.

## LOCAL NOISE ISSUES

Local noise remains a touchy issue for the NASP and any commercial derivatives. Noise during the test program, particularly if the tests take place at a base like Edwards, will probably not pose a great problem. Still, it is practical to be as prudent as possible. Commercial airports strive to keep noise below 102 dB. The NASP JPO should keep this figure in mind as the project progresses.

## SUMMARY

The NASP should not force insurmountable challenges. Still, environmental challenges do exist and will require careful attention. The history of the SST suggests that ignoring them can have consequences far in excess of the actual environmental hazard.

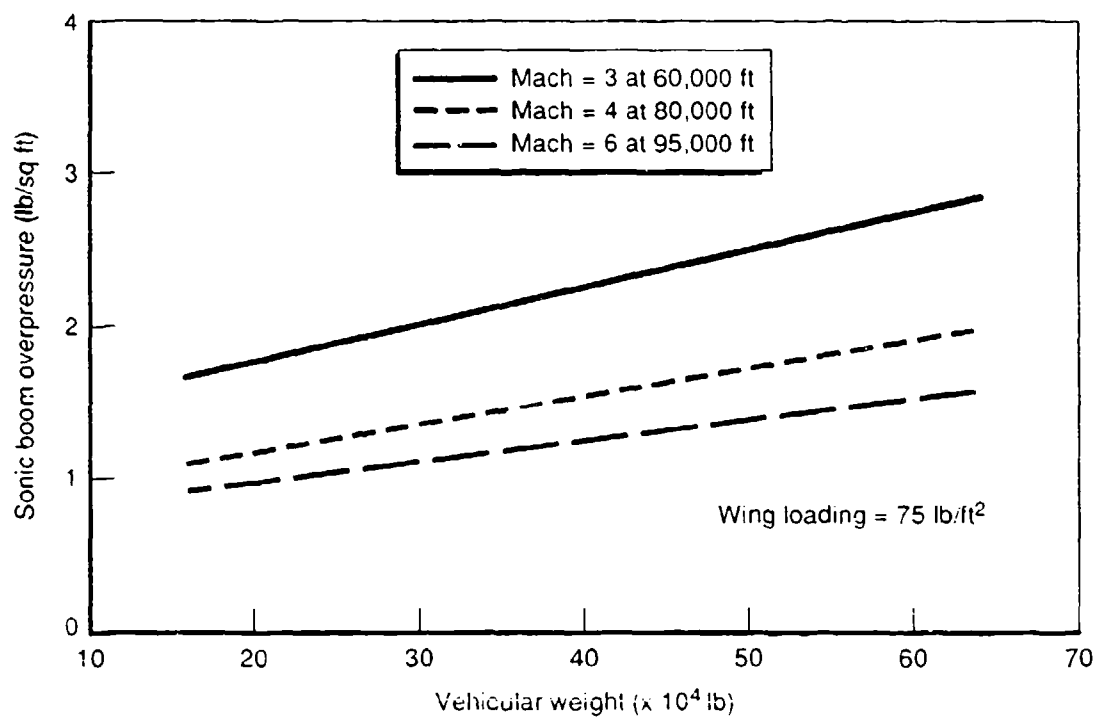


Fig. 5.4—Ground-Level Overpressure for Various Speeds, Altitudes, and Vehicular Weights

## **6. PROGRAM ASSESSMENTS**

### **INTRODUCTION**

The NASP program is now in Phase II, the remainder of which is keyed to major reviews culminating in a possible decision to begin experimental flight vehicle development in early 1993. Clearly, other decisions can be made during this period. Below, we describe some considerations we regard as important to the decision process.

Many things have happened to change the environment in which Phase II began. Strategic programs such as the B-2 and the small ICBM received intense public discussion and redirection. The SDI program has narrowed its focus to space-based small interceptor vehicles. The U.S. ASAT program has abandoned aircraft-launched systems and is considering three alternatives. Unresolved issues surround the future of U.S. space launch. The Administration has proposed a major new Space Exploration Initiative with activities keyed to space station, lunar, and Mars efforts.

On the political scene, concerns about superpower confrontations are waning, while those about Third World countries wax. A growing impetus for arms control exists, with verification and compliance monitoring placing heavy reliance on National Technical Means. And many regard the changing position of the United States in the world community with concern.

Additionally, the United States faces serious budget constraints, which are likely to increase over the next few years. This trend reverses the budgetary growth phase in which the current NASP program was initiated. The combination of these diverse pressures will unquestionably influence the NASP project. Assuming we wish to derive the most effective and enduring U.S. sustained commitment to exploring hypersonic and orbital flight, we should reexamine the NASP program structure in light of today's circumstances. Whatever paths the reexamination takes, the United States should commit to a sustained and intense RDT&E program to explore hypersonic orbital flight, using whatever combination of means judged best suited to meet the objective of moderating the technical and cost uncertainty.

### **PROGRAM REVIEWS**

The NASP program has undergone a number of reviews. The Space Council Review in 1989 provided guidance and redirection. The DSB, AFSB, and RAND reviews focused on specific aspects of the program. These three reviews resulted in a variety of conclusions and recommendations, most of which were in concert. The remainder of this section outlines the guidance for redirection and the conclusions of the review groups. Using the guidance and conclusions as a framework, we recommend some future courses of action for the NASP program.



## **SPACE COUNCIL REVIEW OF NASP ISSUES**

As a result of a DoD decision to eliminate the NASP from the defense budget and the concerns of some senior DoD officials about its management, funding and priorities, the National Space Council reviewed the entire program. Members of the NASP program, on the other hand, felt that changes or wavering of commitment would slow the program or discourage the contractors, who might then shift unique personnel assets to other projects. The review staff brought a variety of perspectives to the task. In June 1989, the Space Council published a draft of their conclusions and recommendations.<sup>1</sup> In part, it concluded:

- NASP benefits the civil, commercial, and national security sectors.
- NASP promotes industrial competitiveness and U.S. space leadership.
- Progress has occurred, but NASP remains technically challenging. A flight vehicle is required to meet program goals, but current technology lacks sufficient maturity to support a vehicle design and development decision.
- Current planning for the experimental vehicle includes both research and future operational objectives. The consideration of operational objectives adds significant complexity and risk to the experimental vehicle program.
- The program should retain a joint DoD-NASA management structure.

Based on these conclusions, the Space Council recommended that the President adopt a policy that:

- Continues the NASP program as a high-priority national effort to demonstrate hypersonic technologies with the goal of single-stage-to-orbit.
- Completes the Phase II technology development program. Develops an experimental flight vehicle after Phase II if technically feasible.
- Focuses on research rather than operational objectives.
- Considers manned and unmanned options, and conducts the program in such a way as to hold technical and cost uncertainties to a minimum.

The Space Council further recommended that the President approve the following actions:

- Retention of joint DoD/NASA management.
- Restructure the technology program to focus only on research and technology objectives.
- Extend the technology development phase approximately 2-1/2 years and plan to begin experimental flight vehicle development in early 1993. Space Council review of the program would precede initiation of vehicle development.

## **DSB AND AFSB REVIEWS**

With the Space Council decisions as a backdrop, we now turn to two other reviews of the program. The DSB and AFSB reviews objectively assessed many aspects of the NASP program.

<sup>1</sup>National Space Council Review of NASP, June 1989.

Both reviews went into considerable depth and contain a wealth of technical commentary and dissections of the many difficult problems NASP must face. Here we highlight some of the points that might influence the structure of an effective RDT&E program to meet the intent of the Space Council decisions.

The DSB delved into the program's technology, raising several concerns. Many of the requisite technologies lacked sufficient maturity to meet the project objectives. In view of the numerous uncertainties surrounding the program, the DSB suggested that the NASP JPO should not adopt a schedule-driven approach but should rather establish a set of technical milestones, each of which would require demonstration before proceeding. It made a variety of recommendations, some of which saw immediate adoption and some which remain unimplemented.

The AFSB report echoed many of the conclusions of the DSB report, but made additional recommendations with respect to an auxiliary rocket system for the research vehicle. Some of the major conclusions of the AFSB were:

- The utility of the NASP is a given.
- Not all parts of the hypersonic flight regime are equally attractive.
- Retaining the option of selecting less than an SSTO vehicle can lessen the risk of the program.
- Several flight vehicles in the research phase might be considered as a means of exploring the complete flight envelope.
- The program needs to strike a balance between incorporating less than fully mature technologies and ensuring sufficient flexibility to assure accessibility of all portions of the flight envelope.

These views were consistent with the earlier work of the DSB, which raised some additional points:

- Adequate closure of the technology maturation phase is needed before committing to flight vehicle decisions.
- Realistic program parameters and concomitant cautions against overly optimistic promises of demonstrable results are essential.
- Experimental exploration of the flight envelope must be separated from mission-oriented follow-on activities.

## THE RAND REVIEW

The RAND review concurred with the eight points raised by the two other review groups while raising several additional points:

- The single-path approach to the project seems less than optimal. The air-breathing SSTO does not appear particularly compelling; it seems simply to be one of several competitive options.
- The NASP has clear potential mission applications, but numerous other alternatives exist that must temper any views of the uniqueness of the NASP.

- Revisiting the early Aerospace Plane program would beneficially broaden the current program's perspective during the RDT&E phase.
- In periods of constrained budgets, a multi-use vehicle provides increased incentives for support.
- An independent oversight committee would be a beneficial adjunct to the program.
- Numerous uncertainties surround the program, and their resolution by 1993 appears highly unlikely.

Taking the points raised by RAND together with those developed by the other review bodies and placing them within the context of the Space Council directions, we formulated a series of recommendations for a redirected NASP program. The sections below outline the general operating context for the NASP program, the broad characteristics of that program, and a specific example of a redirected NASP program. These recommendations point toward a 1993 decision by the Space Council on an array of options that could lead to a further redirection of the program.

### **The Context**

The NASP program should assume a context in which the United States maintains a mainstream aerospace plane role in a time of continued austere budgets. Thus, it should seek as broad a base of applications as possible consistent with prudent applications of technology.

### **Basic Program Features**

An underlying idea of our restructured NASP program would be to use the current technology maturation phase to expand the list of options under consideration. Options would include different SSTO possibilities and different combinations of power plants and two-stage candidates. The program would continue to be a joint DoD/NASA program under Air Force management but would include a standing oversight committee that would provide independent assessments to senior officials on a variety of program aspects, including alternative NASP candidates. The research program would be separate from development of an operational aerospace plane. We would include provision for unmanned and subscale testing.

The program would be formulated with the 1993 review by the Space Council in mind. The council's decision of whether to develop an experimental flight vehicle would be based on a much broader array of options than the air-breathing SSTO alone and would consider more than whether technology was adequate.

The range of decisions the council might address could include:

- A reformulated hypersonic program.
- Formulation of detailed decision criteria for the various options and the circumstances for exercising them. Possible options are:
  - Continue the X-30 program with a longer technology maturation phase.
  - Cancel the program.
  - Maintain a hypersonic proficiency program.

- Establish a hypersonic proficiency program but add unmanned and subscale flight, plus develop some new ground facilities.
- Expand the hypersonic proficiency program by adding a workhorse vehicle to test engine types exploring the Mach 4-8 regime.
- Establish decision criteria for a NASP-like prototype.
- Formulate evaluation criteria and options for access to space after 2005.

We would visualize a "minimum post-1993 hypersonic RDT&E program" as having the following topical elements. Without the detailed analysis we proposed between now and 1993, these elements must be viewed as suggestive and not necessarily in final form.

- Component test
- Continuation of the Rockwell gas facility
- A major new ground facility
- Standard tests of CFD codes
- A continued materials effort

This continuation program would focus on developing new scientific understanding and data, design tools, and enabling technology to underpin high-confidence engineering design capabilities.

Another aspect of future decisions will be the criteria used to judge whether we should proceed with a full-scale manned experimental vehicle like the air-breathing SSTO X-30.<sup>2</sup> A number of milestone accomplishments might be suggested using simple "reasonable man" types of judgments. These include:

- Flight demonstration of a SCRAMJET engine at representative speeds and durations proposed for SSTO.
- Demonstration of guidance and flight management control designs across potential ascent and descent trajectories.
- Full-sized components of all nonqualified new materials.
- Full-sized demonstration of slush hydrogen propellant system.
- Validation of CFD codes using flight data.
- Demonstration of unstart of engines under flight conditions.
- Demonstration of the ability of the SCRAMJET vehicle to maneuver while under thrust.
- Demonstration of instrumentation under flight conditions.
- Establishment of a database for materials costing.

Establishment of such decision criteria could well be one of the functions of the new oversight committee. Although it is true that even under the best of circumstances an air-breathing

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<sup>2</sup>We understand that the current NASP program has developed Phase II exit criteria but we have not had the opportunity to review and compare it with our suggestions.

SSTO X-30 vehicle will have to serve as its own test facility in significant measure, there are clearly degrees of uncertainty that make a great difference in acceptability. Historically, a balance has been struck between the pretest moderation of risk and the acceptable degree of residual risk in committing to manned test. An X-30 of the current type poses this balance in an especially sharp way.

### **Specific Example of an Alternative NASP Option**

Our general proposal is that the 1993 Space Council review several NASP options, and not simply the readiness of the air-breathing SSTO X-30, to proceed toward an experimental flight vehicle. Among the array of possible options, we now discuss one example representative of the alternatives other than the air-breathing SSTO X-30. RAND's opinion is that it is a meritorious contender as a Phase III vehicle.

Our proposal contemplates a new two-stage vehicle: a first-stage workhorse vehicle capable of carrying a second stage. The first stage would be manned, and the second stage could be manned or unmanned. Both stages would be recoverable and reusable.

The two-stage concept would be an experimental flight vehicle designed to test a broad range of concepts to the extent that follow-on vehicles could be high-confidence operational prototypes. As an experimental vehicle, it need not necessarily be the size of any full-size follow-on. It would likely be substantially smaller than the full-size proposed Sanger and might fall in the 100,000 to 250,000-400,000 lb class. It might allow sufficient testing of several concepts to a point where the detailed information base would be adequate to consider going directly to operational prototypes.

Such a vehicle and testing concept mesh fully with the Space Council guidance and the points raised by the DSB, AFSB, and RAND reviews. For example, we do not forgo full exploration of the "ultimate goal of SSTO," and, indeed, a possible outcome of this program is a high-confidence verification of the attractions of an SSTO, air-breathing or rocket, adequate for undertaking operational prototypes. Another possible outcome of the proposed two-stage program is increased opportunities for more research testing; for potential commercial supersonic or low hypersonic speed transports.

### **Oversight Committee**

The increased level of effort and scope of our program redirection suggest formation of a standing oversight committee. This committee, which would operate independently of the NASP JPO, would strongly emphasize to the NASP JPO the importance of considering options other than the air-breathing SSTO alone. It would have JPO representation to ensure that it was up-to-date on the NASP process, but its primary focus would be to ensure development of a broad set of RDT&E options for presentation to the Space Council and other senior officials. It would act in an advisory role with no decision authority independent of the Space Council and senior DoD and NASA officials.

The committee should include individuals with expertise in the technical and programmatic areas of the NASP. The committee would detail the scope and content of NASP RDT&E options for an experimental flight vehicle program meeting the 1989 Space Council guidelines.

Options should include, in addition to the air-breathing SSTO, rocket SSTO and two-stage vehicles with, possibly, a variety of engine types and manned or automatic configurations.

The intent is for this oversight committee to be a working organization supported by the necessary analyses to substantiate their recommendations.

The oversight committee would establish review and action criteria and decision milestones for the evaluation of the several options presented to the Space Council. These items should include tests for establishing technology maturity before proceeding with flight vehicles, compatibility with the findings of the external reviews, and the composition and general utility of associated test programs involving demonstrations and other subscale tests. Criteria established should also allow judgments of the comparative risks among the several options of proceeding to operational prototypes.

The committee should report periodically to the Space Council and other senior officials up to the 1993 decision point. It might remain in operation beyond that point depending upon what decisions the council makes.

## **7. CONCLUSIONS AND RECOMMENDATIONS**

### **CONCLUSIONS**

The major conclusions of our review of the NASP program are:

#### **Mission**

- No single mission justifies NASP.
- The composite mission capabilities of NDVs provide its best rationale, with a small fleet serving the National Command Authority.
- Specific payloads and mission equipment will require a significant RDT&E program.

#### **Technology**

- The present state-of-the-art of CFD will not allow it to serve as a self-contained design tool for NASP, and it is not likely to do so over the next 10 years even with a substantial program of testing, experimentation, and analysis.
- The proposed integrated propulsion system faces unprecedented challenges.
- The present approach to solving these problems allows small margin for error.
- More conservative but less precarious approaches exist, specifically, sub-scale experimentation to complement the basic X-30 program.

#### **Costs**

- Definitive cost estimates are not possible.
- Using airframe costs as a proxy suggests no substantial launch cost savings will accrue from the development and use of NDVs for the space launch mission.
- Given the current high cost of space payloads, a major reduction in launch costs does not seem appropriate as a primary program goal because of the small impact that it will have on the total cost of space operations.

#### **Environment**

- Actual threat to the ozone layer appears minimal.
- Sonic booms and ground aircraft noise will require attention.
- Actual hazards notwithstanding, environmental concerns have substantial potential to affect the program detrimentally if not dealt with carefully.

### **Program**

- The schedule is optimistic.
- The program is focused too narrowly on SSTO; a wide range of alternative approaches needs in-depth analysis.
- The coupling of research and operational objectives increases complexity and may well increase time and costs as well.

### **RECOMMENDATIONS**

Based on these conclusions, we recommend that:

1. The United States commit to a multi-year, high-intensity RDT&E program to explore the hypersonic-orbital flight envelope at a moderate level of funding.
2. The basic structure of the NASP program undergo a reexamination. The resultant structure should include:
  - a. Retention of Air Force management of a DoD/NASA program.
  - b. Consideration of a range of tools for exploration of the hypersonic-orbital flight envelope; the manned X-30 need not be the single linchpin of the program.
  - c. Broadening the program to include options beyond the air-breathing SSTO.
  - d. An event-driven program, with technology maturation as one of the pacing elements.
  - e. Retention in the RDT&E program of selected operational goals as a means of structuring the post-Phase II program.
  - f. Incorporation of lessons of the early Aerospace Plane program into the present one.
3. Creation of a standing, independent oversight committee to ensure consideration of a broad set of RDT&E options for the Phase III decision by the National Space Council.

The NASP program has enormous potential. It can assist in keeping the United States as a world leader in space exploration. It has promise of major technological advances that will benefit the civil and military interests of this country. But realization of this potential will require a careful and prudent approach. We believe the recommendations above will assist such an approach greatly.



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